Florence, 5 *Mars*, 1881

A propos du déplacement projeté des collections botaniques du Musée d'Histoire Naturelle de Florence.

Lorsqu'au mois de Mai de l'année 1874 un grand nombre de botanistes de toutes les parties du monde se trouvèrent réunis à Florence, dans les salles des Collections botaniques fondées par feu le professeur Parlatore, M. le professeur Alphonse de Candolle fit observer "qu'une des choses les plus remarquables s'imposant à l'attention des membres du Congrès, était le Musée botanique, avec ses salles amples et commodes, où avaient lieu les séances du Congrès." ("Actes du Congrès Botanique international, tenu à Florence au mois de Mai 1874," pag. 220.)

Personne, alors, n'eût soupçonné que ce qui avait été jugé digne d'admiration par les hommes les plus compétents, serait déclaré mauvais et condamné à un bouleversement radical sept ans à peine après que ces paroles mémorables avaient été prononcées. Pour justifier l'abandon du Musée actuel, on prétexte l'inconvénient qu'il y a pour les étudiants fréquentant les cours de l'Institut des Études supérieures, place St. Marc, d'avoir à se rendre, pour une autre partie de ces cours, au Musée de Via Romana ; et, afin de centraliser les édifices affectés aux études, on ne pense à rien moins qu'à opérer le déménagement des collections botaniques et à abandonner le jardin du Musée, avec toutes ses serres et annexes. En revanche, on parle de rendre à son ancien usage le modeste Jardin des Simples, situé à proximité du nouvel emplacement destiné aux herbiers. Mais a-t-on examiné si ce transport est réalisable, avantageux, et si le nouveau local de la place St. Marc, destiné à la Botanique, est adapté ou non à recevoir les herbiers et les autres collections ?

Or ce local n'est autre que le bâtiment des anciennes écuries des Grands Ducs de Toscane, occupées plus tard par la cavalerie italienne. Pendant une série non interrompue de près de trois cents années, ces écuries ont logé des chevaux en très-grand nombre, et c'est là que l'on se propose de colloquer des collections de plantes, d'un prix inestimable, et si faciles à se détériorer sous l'influence de l'humidité ! Il est vrai qu'en sacrifiant d'énormes sommes pour reconstruire l'édifice à peu près de fond en comble, on ferait peut-être disparaître les traces du long usage auquel il a servi ; mais il est permis de se demander si, même dans ce cas, on obtiendrait jamais des salles comparables à celles du Musée actuel, soit en beauté, soit en salubrité, soit en solidité.

Ce projet étant soutenu et sur le point d'être mis à exécution par des personnes respectables, mais étrangères à la Science et par conséquent incompétentes, nous Soussignés, amis de la Botanique résidents à Florence, croyons de notre devoir, dans l'intérêt des collections, de protester contre ce déplacement, et, afin de donner plus de poids à notre protestation, nous invitons les Botanistes qui se sont trouvés à Florence lors du Congrès de 1874, ainsi que tous ceux qui connaissent les salles actuellement affectées aux collections de plantes, à joindre leurs voix à la nôtre pour empêcher qu'on ne mette à exécution un projet que nous croyons hautement préjudiciable à nos plus chères études.

Nous prions en conséquence les Botanistes italiens et étrangers de vouloir bien employer leur influence afin que le projet en question soit abandonné, et que les sommes, dès à-présent destinées à une œuvre inopportune et risquée, soient de préférence employées à augmenter le matériel scientifique du Musée actuel, par l'achat de collections de plantes vivantes et desséchées (surtout de plantes cryptogames) et d'ouvrages manquant à notre bibliothèque botanique et à acquérir les armoires et étagères, nécessaires pour placer et mettre en ordre une immense quantité de paquets d'herbier, actuellement sans emploi et inutiles aux études, ainsi qu'à adapter les serres du Jardin botaniqe aux exigences modernes, en commençant par y faire les réparations reconnues de première nécessité.

(*Signé*) A. B. Archibald
D. Bargellini
Odoardo Beccari
Antonio Biondi
Emanuele G. Fenzi
Enrico Groves
Emilio Levier
E. Marcucci
Ugolino Martelli
Vincenzo Ricasoli
Riccardo Ricci
Niccolò Ridolfi
S. Sommier
P. de Tchihatchef

Barometer Pumps

Communications from Mr. Sprengel have been published, in which he has defended his claim to be the inventor of the mercury barometer-pump. As long as he confined himself to this claim I had no right to interfere, but by his letter in your previous number (vol. xxiv. p. 53) he claims to be the inventor or father of all kinds of barometer-pumps. His right to this claim I dispute ; for in May, 1847, I obtained a patent for improvements in sugar-refining, one of which is the conversion of a vacuum-pan into a large barometer by placing under a common vacuum-pan a long pipe in a perpendicular position, which acts as a pump whereby the sugar is taken out of the pan by its own weight in the long pipe, and thereby the vacuum in the pan is not destroyed, and the process of sugar-boiling is carried on continuously. The syrup to be boiled is added in the pan above, while the boiled sugar is taken out below through the barometer-pump. The specification of my patent was published in patent journals in London in 1847, and it is possible that Mr. Sprengel took the idea of his mercury barometer-pump from my sugar barometer-pump. But at all events Mr. Sprengel was not the first inventor of a barometer-pump. I claim that honour.

James Johnstone

Experiment Rooms, No. 1, James Square, Edinburgh,
May 21

The Hutton Collection of Fossil Plants

It has only within the last few days come to my knowledge (indeed only to-day authoritatively) that the Hutton Collection of Fossil Plants, at present deposited in the Museum of the Natural History Society of Northumberland and Durham, at Newcastle, had been named by the curator, Mr. Richard Howse, prior to the compiling by myself of a Catalogue of the Collection, published in 1878 by the North of England Institute of Mining and Mechanical Engineers. The labels on the specimens, referred to in the Catalogue, were therefore Mr. Howse's, and not, as I until now imagined, either William Hutton's original ones, or mere copies of them.

Moreover an unsigned MS. list of the specimens in the Collection, agreeing with the labels, with which I was furnished by the Mining Institute, and which was used freely by me in drawing up the Catalogue, must now be regarded as the result of much time and labour spent by Mr. Howse in identifying and naming the whole of the Hutton Collection.

I trust you will allow me space in your paper to acknowledge now what I should have made a point of acknowledging in the preface to the "Catalogue," had I been made acquainted with the facts of the case at the time. G. A. Lebour

College of Physical Science, Newcastle-upon-Tyne, May 18

"How to Prevent Drowning"

Mr. MacCormac's valuable article induces me to call attention to a prevalent error.

Almost every treatise on swimming tells the beginner that every one can float without exertion. Even Mr. MacCormac seems to imply that "lying quite still with the mouth shut and the head thrown well back in the water" is enough to insure any one against sinking. Now this may possibly be true for most men, but certainly not for all. I am a practised swimmer, fond of the water, and have often tried. Going through all the orthodox motions of the deep breath, the folded arms, and the head thrown back, I go down instantly. This is in fresh water ; in salt I believe I can just float, but have seldom had a good opportunity of trying. The fact is that men are very different in buoyancy. I have seen a man float motionless with head and shoulders out of the water. Others may be even denser than I am. Most men believe themselves capable of coolness and presence of mind. They should remember that these will neither supersede the art of swimming nor alter the laws of gravity.

St. John's College, Cambridge E. Hill

The Effects of Pressure on the Germination and Growth of Plants

The following experiments may be of interest to vegetable physiologists :—On April 7, at 11 p.m., two sets of mustard-seeds—five in each set— were sown on pieces of moist cotton-wool, arranged as follows :—One piece was placed in a small bottle, which was then secured to the curved extremity of a glass tube, into the long arm of which mercury was poured till a height of forty-five inches was reached above the level of the metal in the shorter arm. The second piece, with its seeds, was placed in an exactly similar bottle, the neck of which was then made to dip beneath mercury, the bottle, of course, like the one soldered on to the tube, being inverted. This bottle was then placed beside the first.

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The two sets of seeds were thus in exactly similar conditions, except for the increased atmospheric pressure and the compression of the atmosphere in the one case as compared with the other. The following was the course of development:—By 9 a.m. of the 9th three of the seeds under the 2½ atmospheres of pressure had protruded their radicles, and this protrusion by 12 p.m. of the same day had become considerable, while as yet there was no indication of commencing germination in any of the seeds of the second set. By 10 a.m. of the 10th these latter had just begun to germinate, the radicles of the seeds under high pressure being at the time a fourth and a third of an inch long.

Henceforward, however, the rapidity of development was reversed. The seeds, under ordinary pressure, grew rapidly, and their cotyledons became of a deep green colour; while the development of those under the high pressure became permanently arrested and the cotyledons of one that had entirely escaped from the seed-coats remained as etiolated as though they had been grown in absolute darkness.

They were allowed to remain untouched for eight days, when, as there was no change, the bottle was removed from the tube and simply allowed to stand inverted in the place it had formerly occupied. The two—out of the five—seeds which had hitherto remained unchanged now rapidly germinated, and grew into vigorous green young plants.

Does a greatly increased atmospheric pressure or a greatly compressed air prevent the development of chlorophyll, and while it stimulates germination does it prevent growth?

Liverpool, April 27 WILLIAM CARTER

[This is an interesting observation, and seems to suggest a new and comparatively unworked field of investigation—the effect of different amounts of atmospheric pressure on plant-life. With regard to the decomposition in the presence of chlorophyll and under the influence of sunlight, of carbon dioxide, it is remarked by Dehérain ("Cours de Chimie agricole," pp. 25, 26) that the conditions are analogous to those affecting the combustion of phosphorus. This is not luminous in pure oxygen at ordinary pressure, but becomes so immediately the oxygen is diluted with nitrogen or hydrogen, and still more when the pressure is much diminished. Boussingault has shown that leaves will not decompose pure CO_2 at the ordinary atmospheric pressure; but a small cherry-laurel leaf placed in the pure gas decomposed a cubic centimetre of it at a pressure of ·17m. (*Compt. rend.*, 1865, t. lx. p. 872.)]

The Magnetic Survey of Missouri

IT may interest some of your readers to know that, although our State Legislature absolutely refused to do anything to aid in the Magnetic Survey of Missouri, refusing by a "crushing" vote even to authorise county officers to have a true meridian established, the work will still go on. A gentleman of St. Louis, whose name is withheld at his own request, has assumed the entire expense, and we shall now begin a more minute examination of the Missouri, Grand, and Osage valleys. We shall hereafter travel by wagon, and shall do the work where it is most needed in order to disclose the real directions of the isogonic lines.

F. F. NIPHER

An Optical Illusion

THE illusion described by Mr. Wilson and commented on in an editorial note is anything but a novel one. An apparatus for the experiment was purchased by the Birmingham and Midland Institute, along with a quantity of optical apparatus, from Mr. Robert Addams, in, I think, 1857. Within the last few years I have noticed that the experiment is described and explained in Priestley's "Light and Vision." I am writing from home, or would give the exact reference.

C. J. WOODWARD

Cambridge, May 23

I SHOULD like to know whether the following is a general experience, or only a peculiarity of my own vision?

If I stand with a source of light—a lamp or a window—at one side of my head, so that the light falls strongly on one eye only, and look, successively or simultaneously, at the images of a piece of white paper as seen by my two eyes, the image seen by the eye next the light is greenish white, and that seen by the eye farthest from it is light buff.

If instead of white paper I use the gilt edges of a book, the image seen by the eye next the light is of a beautiful golden green; the other is of a brassy yellow, almost orange.

This phenomenon does not appear to depend on any effect of dazzling, for the experiment succeeds perfectly with very moderate degrees of illumination. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, co. Antrim, May 23

The Speaking Tube Anticipated?

HAS the following appeared anywhere in this connection as yet, or not? If not, please allow it to appear in NATURE with this qualification only, that the italics are mine.

Describing the "speaking trumpets or pipes which ran, we are told, along the whole length of the Wall," Bruce says ("The Roman Wall," by the Rev. John Collingwood Bruce, p. 76), that Drayton long ago sang of them as follows in his "Polyolbion":—

> "Townes stood upon my length, where garrisons were laid
> Their limits to defend: and for my greater aid
> With turrets I was built, where sentinels plac'd
> To watch upon the Pict: to me my makers grac'd
> *With hollow pipes of brasse, along me still they went,*
> *By which they in one fort still to another sent*
> *By speaking in the same to tell them what to doe,*
> *And soe from sea to sea could I be whispered through.*"

Ashton-under-Lyme, May 17 W. CURRAN

J. C. SHENSTONE.—A case of Phyllody of the calyx. "*Ranunculaceæ* particularly liable to this change" (Master's "Teratology," p. 246; recorded in *Anemone nemorosa*, *ibid.* p. 252).

ORIGIN OF THE ENGLISH MILE[1]

IT is known that the mile of 1609 metres long passed among English geographers and navigators as being the length of the terrestrial arc of 1′; in other words they made the degree equal to 60 of these miles. In reality it contains 69·5; there is thus an error of about one-sixth. This error, if it existed long among our neighbours, which I do not know, must have caused many a shipwreck. It has had another very remarkable result; it nipped in the bud the discovery of the law of universal attraction. The first time that Newton's great idea presented itself to his mind the proof failed him, because he made use of the common English mile to calculate the radius of the earth. He renounced the idea for a long time, and only took the calculation up again when he learned the results of Picard's measurement of a degree in France. Whence comes this defective estimate? Certainly it does not proceed from any effective measurement, for the worst degree measurements, among those which have been really made, and not fictitious measurements, like that of Posidonius, are far from presenting errors of such magnitude. English geographers then must have committed some mistake in taking their mile from ancient documents.

So long as navigation was limited to the waters of the Mediterranean, and to coasting along the western shores of Europe, it was scarcely necessary to trouble about the value of this element; but from the time that the discoveries of the Spaniards and Portuguese opened out a much vaster field, sailors were compelled to make some inquiry into the matter. I suppose that the English navigators applied to their geographers, and that these found nothing better to consult than Ptolemy, the great, the only authority in these matters. But Ptolemy himself refers to Eratosthenes; he says that he verified the measurements of the latter and found the same result, viz. 500 stadia for the terrestrial degree. I have thus been led to examine the measurement of Eratosthenes. According to the documents which historians have preserved, Eratosthenes measured the great arc of meridian which separates the parallels of Syené and Alexandria, and finally found 700 stadia to the degree. This is how he worked:—He observed at Alexandria, certainly by means of a gnomon, the zenith distance of the sun at

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[1] Paper read at the Paris Academy of Sciences by M. Faye (*Comptes rendus*, xcii. No. 17).

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